

some years ago, and as the engines referred to were designed many years before then, it is absurd to illustrate them as modern practice when the magnificent creations of Mr. Wilson Worsdell on the North-Eastern and those of Mr. McIntosh on the Caledonian are available.

The compounding of locomotives has also been very seriously considered by many engineers, particularly during the last ten or twelve years. The Webb and Worsdell systems being more or less obsolete, one naturally expected to find the Smith system with three cylinders, which originated on the North-Eastern, engine No. 1619 being the prototype, described and illustrated by one of the recent Midland compounds. Another type of compound represented by the four-cylinder engines on the North-Eastern is conspicuous by its absence.

Of the subject of superheat, which is now being seriously considered by most locomotive engineers, we find no reference in this volume. This is surely a serious omission when there are locomotives running on certain railways in this country fitted with the Schmidt system, a system which claims many economies in working when compared with the heavy boiler expenses involved when working with the high pressures necessary with the compound engine.

Although we have considered it necessary to point out that the author's claim of having brought the third edition of this book up to date has more or less failed, it should be clearly understood that its contents are of a valuable nature, and budding locomotive engineers should obtain a copy without delay. It is certainly one of the best books of its kind. The illustrations are good and the general style excellent.

*Matter, Spirit, and the Cosmos. Some Suggestions towards a Better Understanding of the Whence and Why of their Existence.* By H. Stanley Redgrove. Pp. 124. (London: W. Rider and Son, Ltd., 1910.) Price 2s. 6d. net.

MR. REDGROVE'S theory of matter is that it possesses only a hypothetical reality; we assume its existence only because otherwise the harmony of our individual worlds would be unintelligible. Spirit, he seems to maintain, we know by direct intuition of ourselves—a proposition of great dubiety, if we take spirit in the sense of real substance. But, granting the one a certain, the other a hypothetical objectivity, the objective relation of the two must be determined. Mr. Redgrove holds that God is the ultimate cause of both, spirit the mediate cause of matter. Yet the effect must be regarded as quite distinct and discrete from the cause. It would seem to follow that for God spirit is something analogous to what matter is to us; but this inference is not drawn. Moreover, no reason is given for the ontological subordination of matter to spirit, except the epistemological distinction noticed above; and it might equally be held to prove the ontological subordination of all other spirits to one's own. Mr. Redgrove, indeed, tentatively holds that in telepathy we have direct "sense" of other spirits; but this perception is "symbolic" as well as direct, which means, one must suppose, that it is not truly immediate. The author believes in the immortality of self-conscious beings; but as he also believes that self-consciousness arises out of protoplasmic consciousness by the ordinary processes of the universe, it is not evident why he should assume the impossibility of a relapse into that state. Though these and other difficulties will be met, the book is well worth reading. Mr. Redgrove writes with precision and force, and his discussion is always interesting.

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## LETTERS TO THE EDITOR.

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### The Colour of Water.

I AM reminded by Lord Rayleigh's lecture on this subject of the splendid light-blue colour presented by the tanks of water in which some of the water companies allow the sedimentation to take place of "hard water" which has been treated by Clark's process. I am thinking of those near Caterham and of those at Plumstead. The tanks—to the best of my recollection—are about 20 feet by 40 feet in area and 15 feet deep. The water in the tanks has become perfectly (or nearly) clear, whilst the sides and bottom of the tanks are made brilliantly white by the deposit of calcium carbonate. The intense blue colour is seen at (practically) any angle of vision, and on a sunless, overcast day as vividly as in sunshine. It would be important to ascertain whether the blue colour thus seen is entirely due to the self-colour of the water or whether the phenomenon is in any way due to the minutest white particles of calcium carbonate which are still floating in the apparently clear water, and are acting as do the particles of a blue vapour-cloud. I suppose that it would be an easy thing for a physicist to determine this by the use of a polariscope at the side of the tank.

Also the introduction of black tarpaulin into the tank so as to prevent the reflection of light from the bottom and sides would show whether any amount of blue colour was still exhibited by the water, such colour being then necessarily due to the reflection of light from suspended particles, and not from the sides or bottom. A more satisfactory method would be to draw off (without disturbing the sediment) the blue-looking water into an adjacent tank previously lined with dead-black.

One observation on the colour of water I may venture to record. In a very large porcelain (so-called) bath in a hotel bath-room, where strong sunlight was admitted by a window some 10 feet above the bath, the walls of the room being colourless, I noticed that when the bath was filled to a depth of 20 inches the water had a distinctly blue colour. The porcelain, whilst pure white above, yet beneath the water had a distinctly blue appearance, and the intensity of the colour varied with the movement of the water in waves or ripples. The colour was blue rather than greenish-blue, and this I attributed to the pure white of the porcelain as contrasted with the yellowish tint of enamel. The water in question was that supplied in the Hotel Ritz, in Paris (I think that of the Vanne).

E. RAY LANKESTER.

### The Stability of an Aéroplane.

I HAVE been much interested in reading Prof. Bryan's statement on the subject of the stability of an aéroplane, but I cannot agree with him in thinking that the solution of the problem is to come from the mathematical side. I should be the last to decry the use of mathematics in such a case, but if the final result is to be absolutely trustworthy, there must be no doubtful assumptions made during the process.

It does not appear, from the article written by Prof. Bryan, whether he has taken the viscosity of the air into account, but I presume that he has done so. Perhaps, also, his solutions are only meant to apply to the case of an aéroplane flying in a dead calm. The practical difficulty with a flying machine is the natural wind, and if flying machines are to be of any real use they must be able to maintain their stability in ordinary conditions of weather. Now it is obvious that when a flying machine has once left the ground it is quite immaterial to the stability, provided the air motion is perfectly uniform and steady, whether the velocity of the wind be one mile or one hundred miles per hour, since it is only the relative motion of the machine and the air with which we are concerned; but, as a matter of fact, a wind of from ten to twenty miles an hour is fatal to almost every aéroplane, and the

reason is not far to seek. The air motion known as wind is not even approximately steady and uniform until at least a height of 1000 feet is reached, and on some days there is no uniformity about it until much greater heights are attained. The supposition made, therefore, that the motion is uniform is incorrect, and if this assumption has been made by Prof. Bryan it limits the use of his equations to times of perfect calm.

He has probably also assumed that the flying machine, or at least the separate parts of it, may be treated as a rigid body. The small gliders made of thin mica, and described by Mr. Lanchester, may, no doubt, be treated as rigid, but cloth or canvas forms an important part of an aeroplane, and however tightly it may be stretched it certainly is not equivalent to a rigid sheet, but must yield under varying pressure.

My practical experience on this subject is with kites, and although there are fundamental differences between a kite and a flying machine, there are many points of resemblance, and it is probable that if the question of the stability of a kite could be completely elucidated, that of the stability of an aeroplane would follow. Almost any kite will fly in a steady wind of moderate velocity, but, so far as I know, there is no kite that will remain stable when the wind increases beyond a certain value. The limit with the kites used here is about forty-five to fifty miles an hour at a height of 1500 feet. The question is, Why do they become unstable? There are, perhaps, three reasons. The flying of a kite depends on gravity, for a weightless kite could not fly, as there would be no directive force. As the wind pressures increase, the necessary relationship between the magnitudes of the pressures and the weight is lost, and the kite becomes unstable. A cure, perhaps, lies in increasing the dead weight. Secondly, the stresses produced by the strong wind very likely deform the kite. It is not possible to meet this by increased strength, for increased strength means stronger, and therefore heavier, materials, and hence, unlike a dead weight at the centre of gravity, a greater moment of inertia, which, as I have long known from practical experience, and Mr. Lanchester has shown from the theoretical side, is inimical to stability. Thirdly, it may be that there is a certain critical velocity, like that of water in a pipe, beyond which the motion over the edges of the sails becomes turbulent, and the form of the stream lines suddenly changes.

These points can best be cleared up by extensive experimental work, and I much regret that no such work is being done.

W. H. DINES.

Pyrtton Hill.

It was difficult to prevent my article from running to ten times its present length, and a similar difficulty occurs in this letter. I can only state that the more I have examined the problem of stability the more I have been brought to conclusions exactly the opposite to those expressed in Mr. Dines's letter. My original view was to limit the mathematical investigation to the formation of the biquadratics and their discriminants, leaving their coefficients to be determined by experiment. In several popular articles I have advocated such experiments. Further experience has shown that there was little prospect of any conclusions of a practical character being obtained without a considerable further development of the problem from the mathematical side.

Moreover, experience has led me to believe that the English practical man is sometimes right when he says that if you cannot get a complete solution of a problem you ought to try to obtain the nearest approach to it that you can, and, further, that an admittedly approximate solution is often more useful than a more complicated exact one. In the present case the methods are perfectly general, and while it would have been futile to start, in the first instance, by making the problem too complicated, there is little difficulty in introducing any necessary modifications subsequently.

In regard to the assumption of uniform relative velocity, surely Mr. Dines is confusing the issues between stability and something else. The very notion of *stability* implies the existence of a state of equilibrium or of steady motion. The behaviour of an aeroplane or kite in a turbulent

atmosphere is a problem, not of stability, but of forced oscillations. The effect of these oscillations is to modify the motion of the kite; but it is necessary in the theory of forced oscillations to assume as a standard of comparison the state of equilibrium or steady motion which would exist if the disturbing forces were absent, and the character of the forced oscillations will depend vitally upon whether this state of equilibrium is stable or unstable. The first thing is to investigate the free oscillations, but there is nothing to prevent the investigation extending to forced oscillations as well. It is only a question of time and opportunity. Meanwhile, aviators are performing record flights on aeroplanes which do not satisfy the conditions of stability, and Mr. Dines's turbulent air currents are getting blamed for much that is not their fault. No solution of the problem can be final which does not completely separate the effects of free and forced oscillations. I do not see how this can be done by purely experimental methods of investigation, and surely the questions which Mr. Dines raises in connection with the behaviour of kites refer to this very point. He has, in fact, stated another problem for the mathematician.

Again, like Mr. Dines, I was at first of opinion that it might be possible to establish a simple connection between the stability of an aeroplane and that of a kite, and I suggested this problem to Mr. Harper. The investigation turned out to be more complicated and less likely to lead to any practical conclusions than was originally anticipated. With so many other problems on hand, it appeared desirable to give this one up for the present. I do not think Mr. Dines will find it possible to obtain an experimental solution. If he can, a great deal of trouble will be saved; but so many variables enter into the problem that it appears to me necessary to start with some idea as to what the connection is likely to be, *i.e.* with some kind of a mathematical theory as a starting point.

When Newton published his "*Principia*" it would have been an easy matter for a forerunner of Mr. Dines to raise objections to this attempt to solve problems relating to moving bodies by applications of the laws of motion on the ground that the motions must largely depend on air resistance, friction, the earth's rotation, and other causes, and that a solution which did not take all these effects simultaneously into account could be of no practical value. Similarly, it might have been objected that the solution of the problem of stability of ships could not be approached by means of the theory of the metacentre on the ground that it failed to take account of wave motion, the velocity of the ship, the effects of the wind, and such resistances as viscosity. I venture to suggest that if the above problems had been attacked by purely experimental methods alone, we might still have been in the dark as to their solutions.

In my article I alluded to difficulties caused by "want of opportunity," but out of fairness reference ought to have been made to the similar difficulties referred to in Mr. Lanchester's book. Had his investigations been published when they were made, a large amount of work might now have been accomplished which is still undone.

It is only fair also to add that since the appearance of the note I have had some correspondence with the editor of the *Aëronautical Journal*, and I hope he will succeed in maintaining the same high standard in its published papers of a mathematical character as in its illustrated articles on general aeronautics.

G. H. BRYAN.

#### Colour-Blindness.

IN the issue of NATURE of January 27 there is a discussion of colour-blindness, with particular reference to the case of Mr. John Trattles, whose colour-vision seems to have been difficult to determine. It is not my purpose to discuss the peculiarities of this case, or the methods of examination used by the Board of Trade. As a worker in this branch of science, however, I may be permitted to say that in any case of supposed defective colour-vision there need be no appeal from the opinion of Sir William Abney, who is so justly celebrated on both sides of the Atlantic for his researches on the perception of colour.

Since, however, in the case of Mr. Trattles, and possibly in others as well, the evidence was not regarded as con-